Review 1 – Author Response

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The following section provides a point-by-point response to Reviewer 1 (Anonymous). The reviewer's comments are shown in black, followed by our response shown in blue.

1. General Comments

This study focuses on investigating how ice shelf fronts on Porpoise Bay (Holmes East/West, Frost and the creatively named Glacier 1) have evolved since the late 1990s (or the early '60s for ice shelf front) by utilizing a mix of satellite imagery, radar interferometry alongside previously published data on the state of the ice shelf front. This shelf evolution was compared to temperature/salinity data taken in Porpoise Bay from MEOP casts and EN4 reanalysis data to provide evidence of any continental deep water intrusions onto the shelf front, which could support heightened basal melt. The authors also find a previously undocumented calving event on the Holmes East glacier and link the breakup of the protected mélange layer to a loss of local sea ice in the months leading up to calving.

Overall, I found this a well written and interesting manuscript on what is both an underrepresented region in literature, and a difficult region to analyze owing to the aforementioned presence of mélange. With a bit of tightening, it should be a very useful paper for anyone looking to undertake research in the region. Also, low-key love the care put into colour schemes throughout. We thank Reviewer 1 for their positive feedback on the manuscript, and we are pleased to note that we are able to address the vast majority of their comments (see detailed response below).

2. Specific Comments

Line 20: following the break-out of multi-year sea ice alongside a recent calving event., further… "Alongside" will be added to this sentence.

Lines 30—35: Seeing there have been a few big mass balance papers out recently, I'd be curious to see some comparison to Green et al., 2022 (doi: 10.1038/s41586-022-05037-w) and Davison et al. , 2023 (10.1126/sciadv.adi0186). Since these papers discuss ice shelf mass balance, we will add them to the Line 40 to bring in the point that ice shelves lose mass from calving and thinning.

Lines 43-45: The start of "However, there is emerging evidence of a longer-term trend in Wilkes land seems at odds with the previous sentence that Wilkes land is the

dominant source of mass loss in EAIS. “However” will be removed as suggested and replaced with “Indeed.”

2.2 Ice surface elevation general comment 1: I found the repeated usage of “the data” or “this dataset” made this section hard to follow. This section will be reworded to reduce the use of “data” and “dataset” to instead refer to the data by their author, as suggested.

Ice surface elevation general comment 2: I wonder if it would flow better if the Nilsson et al., part is given its own paragraph, plus a tad more information on the additional data from ICESat-2 and how it meshed with the previous data. The Nilsson et al. dataset will have its own paragraph with more information on ICESat-2.

Line 161: I’d like the error range given for each box instead of the range. Especially seeing that is a rather large difference in error. Instead of providing 12 error ranges in the text (3 boxes for each of the four glaciers), which may be dense, we propose to add an additional table into the Supplementary Materials (see below) that display the annual error measurements for all four glaciers across the three box types (IN, GL, FT). This also resolves additional comments from other reviewers. We will refer to this in Line 161 for clarity and elsewhere in the manuscript.

Year	IN velocity error (m yr⁻¹)				GL velocity error (m yr⁻¹)				FT velocity error (m yr⁻¹)			
	FR	G1	HE	HW	FR	G1	HE	HW	FR	G1	HE	HW
2000	110.8	-	-	-	147.1	40.0	-	100.2	98.3	179.3	-	-
2001	55.6	86.8	-	-	81.9	45.8	-	196.2	53.5	65.1	-	-
2002	-	-	-	-	-	40.2	-	37.8	48.9	-	-	-
2006	-	-	-	-	-	-	-	183.7	412.8	345.1	-	-
2007	35.9	50.6	-	-	13.8	10.8	-	22.0	10.1	48.9	-	-
2008	86.7	64.3	-	-	36.1	19.5	-	36.9	25.4	75.4	-	-
2009	150.1	101.8	-	-	47.5	30.0	-	53.4	27.0	95.4	-	-
2010	95.8	68.5	-	-	30.4	33.8	-	78.4	19.1	90.4	-	-
2011	68.6	75.6	-	-	29.3	30.9	-	45.9	23.0	78.4	-	-
2012	-	-	-	-	63.0	62.2	-	56.6	62.0	-	-	-
2013	24.8	25.1	36.8	158.1	23.4	18.1	23.9	33.2	20.9	22.4	13.8	15.3
2014	4.3	5.1	11.4	40.3	2.7	2.1	11.5	4.3	2.4	2.3	7.4	6.6
2015	2.5	2.9	4.3	6.1	2.6	1.6	3.9	3.0	2.2	1.9	3.5	3.3
2016	2.0	3.8	6.4	10.9	2.1	0.8	7.2	4.1	1.5	1.3	5.1	8.1
2017	3.6	4.4	13.0	15.3	2.8	1.5	10.3	3.9	2.3	2.9	5.3	5.2
2018	6.3	7.9	96.1	57.3	4.4	2.1	57.2	6.1	3.8	3.7	8.4	6.8
2019	58.1	112.4	123.1	93.1	36.2	48.5	100.2	81.6	39.8	69.9	95.3	152.0
2020	49.4	181.9	220.7	104.0	28.8	54.3	106.3	85.2	51.6	70.0	209.2	240.4
2021	37.0	103.5	108.2	140.0	36.3	43.6	122.2	76.1	35.4	52.1	167.9	163.9
2022	152.5	107.4	121.9	105.5	29.5	43.9	107.4	50.6	43.9	86.6	212.1	185.7

Line 178: Uncertainty was derived from… “By” will be replaced with “from.”

Line 246: Considering the mention of ice-shelf breakup, I’d also mention grounded icebergs if we’re going for potential “errors” in NSIDC sea ice index. “Grounded icebergs” will be added to line 145.

Line 255: Fluctuated from where (especially as you abandon this kind of measure in the next paragraph to go relative from calving)? Across the study period, the ice shelf length change varied from 5.7 km (minimum - Frost) to 11.3 km (maximum - Glacier 1). We have indicated that this is relative to the 1963 position.

General result comment: isn’t slowed down by – number a double negative? I think the results would be improved by sticking to either a sign to indicate change, or text. Great spot – we will resolve all the double negatives by either describing the retreat as a “change” or removing the +/– signs.

Line 286: There is an additional space after 382.3. Great spot, we will remove this.

Line 291 consistently increased by 7-10% over the study period would make it more concise. Same comment on Line 295. The wording here will be amended in line with this suggestion.

Ice shelf elevation change: I found this section awkward in its layout, and I think it would benefit from figures 5 and 6 being combined (as much as they're pretty figures). Alternatively, I wouldn't be opposed to text, figure 5, figure 6 as the layout. The layout will be changed to read text, Figure 5, Figure 6 to improve flow – combining the two figures would either make them too big for a single page figure, or we would have to shrink panels too much and thus the detail would become unclear.

Line 334: The "we observed lower rates…" in a paragraph that continuously reminds the reader that this data is from other sources seems out of place (not that I'm against the we observed, it just came off as jarring). This will be changed to read "Lower rates of surface elevation loss were observed at…."

Line 365: I'm not a fan of "taken at face value". It's a given with results. This will be removed, although we will remain clear about the uncertainties with the grounding line data, which are important.

Figure 8 and 9: Considering how much these figures interplay with each other, could they be combined? For page layout and clarity, we are concerned that this would make the figures too small to read, so we propose to keep Figures 8 and 9 separate, if possible.

Line 435: This feeds into a comment I have in the discussion, but how does a February-March combination look for concentration anomaly? Seeing they are typically the two lowest months for sea ice extent. We plotted a 6-month seasonal average to demonstrate that Porpoise Bay experienced low sea-ice conditions during the 6 months before each calving event (Figure 14), as well as a monthly sea ice concentration plot (Figure 13). Figure 13 shows each month throughout our analysis period and already demonstrates that the March sea ice concentrations are significantly lower than the February ones. This indicates that doing a mean of the Feb-March concentration would not be helpful. We will further clarify what Figure 13 shows (i.e., monthly change through time) in line 431, to highlight that the March concentrations are considerably the lowest.

Figure 13: Can you make each tick visible. We will amend Figure 13 to make all ticks visible.

Figure 14: Can you align the ticks to their respective bar. Took a few seconds to get my bearings with this one. We will align the ticks in Figure 14.

Line 449: Holmes West glacier are consistent with dynamic … "That" will be removed in line with this suggestion.

Line 456: ice velocity is increasing across … "Also" will be removed.

Line 476: I'm not sure "down a retrograde slope" is necessary. "Down a retrograde slope" will be removed in the revised document.

Line 485: retreat could be less rapid than we record. “Have been” will be replaced by “be” in line with this suggestion.

Lines 511-515: I think that would benefit being broken over two sentences. We will split this sentence in the middle.

Line 562: glaciers (Fig 8.) alongside the rapid decrease in surface elevation (Fig 6d)., since Holmes … This will be amended to read as suggested.

4.3 Role in sea ice driving ice shelf and glacier flow change general comment: I found the lack of a mention for the Voyeykov an odd decision here, owing to the similar mélange break up and proximity to Wilkes Land. This is a good suggestion, and we will now mention the Voyeykov study explicitly in Line 572 by mentioning the study by Arthur et al. (2021).

Line 571: I’d switch Baumhoer et al., 2021 for Teder et al., 2025 (doi: 10.1038/s41561-025-01713-4). Bit of a double whammy where Baumhoer doesn’t go into great detail about fast ice/melange, and Teder et al., does give evidence for a very large calving event being in response to mélange breakup (seeing the perennial fast ice in north Wilkins is a mélange layer). Great idea, Baumhoer et al., 2021 will be swapped for Teder et al., 2025 in the revised document.

Line 577: This does feedback to the comment earlier about being curious about the February concentration and combining the sea ice low period. Considering Teder et al., + Arthur et al., showed evidence that there was a persistently anomalous sea ice low in the couple of months leading up to the breakup of mélange, I’d be curious how well this breakup compared to those two regarding timing. Figure 13 depicts monthly sea ice data through time. We mention March in the text to highlight that in March there are significant drops in sea ice concentration that do not occur in February. Thus, we see no evidence of sea ice lows here in the run-up to March when the concentrations suddenly reduce significantly. We will clarify in the text that Figure 13 is a monthly record rather than a March record.

Line 590: With the bulldozing comment, its certainly possible seeing fast ice growth tends to be limited by depth around Antarctica (Fraser et al., 2023, doi: 10.1029/2022RG000770). That said, I’m not sure how feasible it would be to provide bathymetry of where this mélange was pushed too. It is not quite clear what the reviewer is suggesting might need to be amended, but we are happy to remove the comment about the bulldozing and leave in the citation to Miles et al (2017). We would also note that bathymetry data is either scarce or very unreliable around Porpoise Bay.

Lines 605—610, seeing proximity, how does this compare to what has been observed on the Totten ice shelf (Green et al., 2018, doi: 10.5194/tc-12-2869-2018). Great link. Our results show a similar trend to Totten, and this will be mentioned in Line 612 citing the Greene et al. (2018) study.

Line 660: we report on a previously unidentified calving. “New” will be removed in line with this comment.

Personally, I’m not a fan of the abbreviated journal titles, though if this is the cryosphere template, please disregard. This is the Cryosphere template.

Admundson et al., becomes justified in its second line (and there are a couple of others in the reference list). All the references are formatted as per the Cryosphere template.

Favier, L. has F. Pattyn instead of Pattyn, F. Great spot! This will be corrected.

Haran et al., 2021 has a duplicate entry. This is not a duplicated entry; it is two very similar citations for two datasets produced by Haran et al. in 2021 (MOA2004 and MOA2009).